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REV. HUMPHREY LLOYD, D. D., President, in the Chair.

The Rev. Dr. Robinson presented an autograph of Euler.

Dr. Apjohn drew the attention of the Academy to some researches in Thermo-chemistry, with which he has been recently occupied.

"It is well known" (he observed) "to chemists, that muriatic acid gas and ammoniacal gas are absorbed by water in large quantity, and that during their absorption much heat is developed. The experiments which I have undertaken had for object, to submit to exact estimation the heat so evolved; and the results being, as far as my information extends, quite novel, and, I think I may add, theoretically and practically interesting, I am anxious to present them to the public with as little delay as possible.

"The apparatus employed in these experiments, though necessarily somewhat complex, proved admirably adapted to the purpose to which it was applied. It consisted of a flask, in which the gas was developed, of an exsiccation tube, and of a pair of tubes of small bore attached to the latter, by one

of which the gas, when dried, could be conveyed into a cup containing mercury covered with a stratum of water, and, by the other, into a small cylinder of very thin copper containing the liquid to be heated. The cylinder was furnished with two necks, into one of which a very delicate thermometer was fitted by means of a cork, while the second received the tube by which the gas was to be introduced. By turning a stopcock attached to the latter tube, the gas could at any instant be conveyed into the copper cylinder, or excluded from it, an assistant at the same moment raising or depressing the vessel containing the mercury, so as to prevent or permit of the issue of the gas at this part of the apparatus. The temperature of the air at the instant of the admission of the gas, and the temperature of the liquid in the cylinder, having been accurately noted, and, in addition, the time m, which elapsed between the introduction and exclusion of the gas, as also the time m', which intervened between the latter manipulation and the second reading of the thermometer, data were obtained for calculating the total rise of temperature, and hence, for estimating the heat evolved by the gas as a consequence of its absorption. Thus, if n be the number of degrees by which w grains of water are heated, by the absorption of G grains of gas, $\frac{nw}{G}$ will be the heat extricated, i. e., the number of degrees that the caloric given out by the gas would heat an equal weight of water.

"Such is an outline of the general course pursued. The experiments required the greatest attention, with a view to the management of the apparatus, and the accurate mensuration of time and temperature; and I am not a little indebted to my scientific friend, Dr. Head, for the valuable assistance which he has rendered to me.

"The observations being made, other difficulties remained to be overcome. How, it will be asked, is the true value of n to be determined? For the observation of temperature

made at the close of the experiment is obviously less than the truth, for two reasons: 1st. Because it was not taken until m' minutes after the gas was cut off. 2nd. Because, during the m minutes that the gas was being absorbed, the copper cylinder was hotter than the surrounding air, and, therefore, constantly losing heat. After a good deal of reflection on the subject, it finally occurred to me to adopt the following method of calculating the necessary corrections.

Having introduced into the copper cylinder, furnished with its thermometer, 4.66 cubic inches of water, very nearly the quantity used in all the experiments, and raised the whole to 93.4° (the air being 53.7°), the temperatures were noted at intervals of a minute, until the thermometer indicated 66.5°. To those I then applied the expression for the velocity of cooling, deducible from the Newtonian law, viz.:

$$v = \frac{\mathrm{T}(h.l.\mathrm{A} - h.l.\mathrm{T})}{t},$$

A being the excess of temperature of the cylinder over the air at any instant, and T the excess after t minutes, and thus obtained the velocities of cooling corresponding to the successive values of T separated by intervals of a minute. These being reduced to a tabular form, furnish, by mere inspection, the means of applying the first of the two corrections already indicated, or of ascertaining the temperature which the thermometer would show, had it been read at the instant the gas was cut off, or m' minutes previous to the actual time of observation.

" But the rise of temperature actually produced is less than that which we are in search of, in consequence of the cooling power exercised by the joint influence of radiation and atmospheric contact during the time m. The method I have adopted of determining the effect of refrigeration, and which, as far as I am aware, has not been previously used, I shall now explain.

" The velocity of cooling at any instant while the gas is

passing in, is, adopting the Newtonian law, proportional to the rise of temperature at such instant. But the gas having been always introduced in my experiments at a uniform rate, the rise of temperature is proportional to the time. Hence the velocity of cooling at any instant is proportional to the time. Such being the case, the well-known theorems, which relate to the motion of a material point actuated by a constant force, are here strictly applicable; and, amongst the rest, that the space (number of degrees) through which the cylinder cools in the time *m*, is equal to half the rectangle under the time and the last acquired velocity. This theorem, in fact, immediately gives the correction in question, not, I may observe, in an approximate, but in a complete manner, and, in practice, I have every reason to be satisfied with it.

"In what precedes it will be seen, that I have employed the Newtonian law of cooling, which the researches of Dulong and Petit have shown not to represent observations with rigour, except when the excesses of temperature are small. My results, however, are not on this account appreciably less accurate, for the thermometer which I employed only read to tenths, and the divergence of the Newtonian law from the truth, within the range of my experiments, is only observable in the second decimal place.

"Having explained every thing necessary to enable the Academy to judge of the accuracy of my results, I shall now state the numbers at which I have arrived:

	Equal weights.	An atom.
Ammoniacal gas passed into water,	940°	940°
Muriatic acid gas passed into water,	885°	1900°

Weight for weight, then, ammonia gives out more heat than muriatic acid; but an atom of the latter gives out almost exactly the double of the heat evolved by an atom of the former.

"The number for ammonia, it will have been observed, does not materially differ from that for aqueous vapour of maximum density at 212°, the latter having been fixed, by the

recent experiments of Brix, at 972°. The numbers, however, are not strictly comparable. For, the heat evolved by the steam is truly its latent heat, or is due solely to its change of state, while the caloric evolved by ammoniacal gas, or muriatic acid gas, undoubtedly consists of two distinct parts, viz., of the heat of *compression* of these gases, and of that due to the chemical action exerted between them, supposed in the liquid condition, and water.

"Though I did not entertain any doubts as to the accuracy of the results just stated, it was obviously desirable to resort to some experiments, if any such could be devised, by which they could be tested; and none appeared better suited to the purpose, than to pass ammoniacal gas into liquid muriatic acid, and muriatic acid gas into liquid ammonia, and determine, by the means already explained, the heat developed in each case. Such experiments were accordingly performed, care being taken that the gas introduced did not in amount exceed what would be necessary for saturating the opposite principle contained in the liquid, and subjoined are the numbers to which they have conducted:

Heat of ammoniacal gas passed into liquid muriatic acid,	2523°
Heat of muriatic acid passed into liquid ammonia, . .	1527°

Deducting from the former 940°, and from the latter 885°, the remainders are 1583° and 642°. Now, according to Andrews (Transactions, Royal Irish Academy, vol. xix. part 2), .129 of a gramme of ammonia, in the form of aqua ammoniæ, in combining with liquid muriatic acid, evolves sufficient heat to raise 31.09 grammes of water 5.58°, from which it is easy to calculate that it would raise an equal weight of water 1344°. But the heat evolved by equal weights of ammonia and muriatic acid, in combining with each other, are obviously reciprocally proportional to their atomic weights, so that, 1344 being the number for ammonia, $1344 \times \frac{17}{36.5} = 626$ will be the number

for muriatic acid. The following, therefore, are the comparative results, the numbers in first column being the *differences;* those in the second, the heat of the chemical action between aqueous ammonia and muriatic acid, as inferred from the experiments of Andrews; and those in the third, the excesses of the former over the latter:

	Differences.	Andrews.	
Ammoniacal gas into aqueous acid, .	1533°	1344°	239°
Muriatic acid gas into aqueous ammonia,	642°	626°	16°

"A glance at these numbers is sufficient to show that, when muriatic acid gas is passed into aqueous ammonia, the heat extricated exceeds that obtained when the gas is passed into water, by almost exactly the heat of the chemical action of aqueous muriatic acid and ammonia; while, as respects the case of ammoniacal gas passed into water and aqueous acid, this equality is wanting, the estimate by difference exceeding the direct determination by 239°.

"This is a very curious result, and I was so startled by it, that it was not my intention to give publicity to these experiments until I had more frequently repeated them, and executed others, which I had planned with the view of throwing additional light upon the subject of my inquiry. In entering, however, upon this new investigation, I had the misfortune to lose, by an accident, both my thermometers, and as I do not anticipate being able to return to it for a considerable time, I gladly avail myself of the permission of the Council, to submit these researches, in their present state, to the judgment of the Academy. I entertain, indeed, a very confident hope, that the numbers at which I have arrived will eventually be found to be very close approximations to the truth. Assuming such, for a moment, to be the case, and, in addition, that the results of Dr. Andrews, in relation to the heat arising from the chemical action of aqueous ammonia and aqueous muriatic acid, are rigorously correct, it has occurred to me that my results admit of the following interpretation:

"Let $a + b =$ heat given out by ammoniacal gas when absorbed by water, a representing the heat of *compression*, and b that of the chemical action between compressed or liquid ammonia and water. When ammoniacal gas is passed into liquid muriatic acid, the heat represented by b will be wanting, and that actually developed will be $a + c$, c being the chemical heat determined by Andrews. The difference of these, therefore, or $a + c - (a + b)$, will be $c - b$. But this difference we have actually found to be greater than c. b must, therefore, have a negative sign; or, in other words, when *compressed* ammonia is brought into contact with water, *cold*, not heat, is the result.

"This may appear a very paradoxical supposition, but I am not aware of any fact which would prevent us from entertaining it; and the great expansion which water experiences when absorbing ammoniacal gas, even confers upon it some degree of probability. I may add, that this view of the matter gives us 239° as the value of b, and suggests an experiment, which, though difficult, it would not be impossible to perform, and the result of which would at once elucidate completely the subject under consideration."

The Rev. Dr. Todd exhibited an ancient Irish brooch, belonging to the Rev. Richard Butler, of Trim.

Mr. Petrie having been called on for his opinion respecting the style, workmanship, and age of this beautiful relic of antiquity, stated, that he considered it as the most elegant specimen of Irish workmanship *in silver* which he had hitherto seen, but believed its age to be not so great as that of most, or perhaps any, of the brooches in the Museum of the Academy, or the other collections in Dublin; its minor ornaments being peculiarly those characteristic of the early portion of the twelfth century, to which period he referred it; though